M. Van der Waals makes his molecules elastic spheres, which, when not in contact, attract each other. His treatment of the "molecular pressure" arising from their attraction seems ingenious, and on the whole satisfactory, though he has not attempted a complete calculation of the attractive virial in terms of the law of force.

His treatment of the repulsive virial, however, shows a departure from the principles on which his investigation is founded. He considers the effect of the size of the molecules in diminishing the length of their "free paths," and he shows that this effect, in the case of very rare gases, is the same as if the volume of the space in which the molecules are free to move had been diminished by four times the sum of the volumes of the molecules themselves. He then substitutes for V, the volume of the vessel in Clausius' formula, this volume diminished by four times the molecular volume, and thus obtains the equation—

$$\left(p + \frac{a}{V^2}\right)(V - b) = R(1 + at),$$

where p is the externally applied pressure, $\frac{a}{V^2}$ is the molecular pressure arising from attraction between the molecules, which varies as the square of the density, or inversely as the square of the volume. The first factor is thus what he considers the total effective pressure. V is the volume of the vessel, and b is four times the volume of the molecules. The second factor is therefore the "effective volume" within which the molecules are free to move.

The right hand member expresses the kinetic energy, represented by the absolute temperature, multiplied by a quantity, R, constant for each gas.

The results obtained by M. Van der Waals by a comparison of this equation with the determinations of Regnault and Andrews are very striking, and would almost persuade us that the equation represents the true state of the case. But though this agreement would be strong evidence in favour of the accuracy of an empirical formula devised to represent the experimental results, the equation of M. Van der Waals, professing as it does to be derived from the dynamical theory, must be subjected to a much more severe criticism.

It appears to me that the equation does not agree with the theorem of Clausius on which it is founded.

In that theorem p is the pressure of the sides of the vessel, and V is the volume of the vessel. Neither of these quantities is subject to correction.

The assumption that the kinetic energy is determined by the temperature is true for perfect gases, and we have no evidence that any other law holds for gases, even near their liquefying point.

The only source of deviation from Boyle's law is therefore to be looked for in the term $\frac{1}{2}\Sigma\Sigma(Rr)$, which expresses the virial. The effect of the repulsion of the molecules, causing them to act like elastic spheres, is therefore to be found by calculating the virial of this repulsion.

Neglecting the effect of attraction, I find that the effect of the impulsive repulsion reduces the equation of Clausius to the form—

$$pV = \tfrac{1}{3}\Sigma(mv^2)\left\{1 - 2\log.\left(1 - 8\frac{\rho}{\sigma} + 17\frac{\rho^2}{\sigma^2} - \&c.\right)\right\}$$

where σ is the density of the molecules and ρ the mean density of the medium.

The form of this equation is quite different from that of M. Van der Waals, though it indicates the effect of the impulsive force in increasing the pressure. It takes no account of the attractive force, a full discussion of which would carry us into considerable difficulties.

At a constant temperature the effect of the attractive virial is to diminish the pressure by a quantity varying as the square of the density, as long as the encounters of the molecules are, on the whole, between two at a time, and not between three or more. The effect of the attraction in deflecting the paths of the molecules is to make the number of molecules which at any given instant are at distances between r and $r + dr$ of each other greater than the number in an equal volume at a greater distance in the proportion of the velocities corresponding to these distances. As the temperature rises, the volume being constant, the ratio of these velocities approaches to unity, so that the distribution of molecules according to distance becomes more uniform, and the virial is thus diminished.

If there is a virial arising from repulsive forces acting through a finite distance, a rise of temperature will increase the amount of this kind of virial.

Hence a rise of temperature at constant volume will produce a greater increase of pressure than that given by the law of Charles.

The isothermal lines at higher temperatures will exhibit less of the diminution of pressure due to attraction, and as the density increases will show more of the increase of pressure due to repulsion.

I must not, however, while taking exception to part of the work of M. Van der Waals, forget to add that to him alone are due the suggestions which led me to examine the theory of virial more carefully in order to explore the continuity of the liquid and the gaseous states.

I cannot now enter into the comparison of his theoretical results with the experiments of Andrews, but I would call attention to the able manner in which he expounds the theory of capillarity, and to the remarkable phenomenon of the surface tension of gases which he tells (p. 38) has been observed by Bosscha in tobacco smoke. As tobacco smoke is simply warm air with a slight excess of carbonic acid, carrying solid particles along with it, the change of properties at the surface of the cloud must be very slight compared with that at the surface where two really different gases first come together. If, therefore, the phenomenon observed by Bosscha is a true instance of surface-tension, we may expect to discover much more striking phenomena at the meeting-place of different gases, if we can make our observations before the surface of discontinuity has been obliterated by the inter-diffusion of the gases.

J. CLERK-MAXWELL

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

An Anagram

THE practice of enclosing discoveries in sealed packets and sending them to Academies, seems so inferior to the old one of

Huyghens, that the following is sent you for publication in the old conservated form :—

$A^8C^3DE^{12}F^4GH^6I^6L^3M^3N^5O^6P$
$R^4S^5T^{14}U^6V^2WXY^2$ WEST

"Manufactured Articles"

THERE are precedents to justify a hope that it would be no excursion beyond the province of NATURE, if somebody who knows that molecules possess the essential character of a manufactured article were kindly to explain how he knows a manufactured article when he sees it, in his mind's eye or elsewhere.

The answer used to be "contrivance, design; an end, a purpose; means for the end, adaptation to the purpose." This, it was said, we find in a watch; "we perceive that its several parts are framed and put together for a purpose." The same thing, it was further said, we find still more in the works of nature, "and that in a degree which exceeds all computation." And why so much more? Because "the contrivances of nature surpass the contrivances of art, in the complexity, subtlety, and curiosity of the mechanism; and still more, if possible, do they go beyond them in number and variety." This was the old answer: the new one is contained in such phrases as these: "exact equality," "exact unison," "exactly the same magnitude," "constants not approximately but absolutely identical."

Here it is hard not to stop and ask what can possibly prove that these things are absolutely so: or what can possibly contribute the smallest probability to a hypothesis that anything is absolutely anything, I do not say among the laws of nature, but among its collocations. Very likely it might be proved that the mean-square variation in the value of one of the above-mentioned constants is a prodigiously smaller fraction of its mean value than any other fraction which the molecular theory has occasion to take account of; and anyhow the fact remains that a molecule of bismuth, for instance, differs from a molecule of lead immensely more than two molecules of either can differ from one another. Perhaps this will do as well for the argument; if so, there is no excuse for the absolute; and whether or no, the argument will be the better for explanation, or perhaps it will be the worse for the argument.

However this may be, the difference between the old answer and the new one is rather instructive. An eager disputant might say the new one was contradictory of the old one; but it is safer to say that the new is at best independent of the old. Clearly a watch is about the last thing which would be cited to illustrate the new sort of manufactured article. The examples which our authors do by preference cite are coins, weights, and measures; and certainly it would be difficult to name manufactured articles which should better exemplify uniformity for the sake of uniformity. And for a very good reason (that is the worst of it); because the purposes of coins, weights, and measures are defeated, they who handle them deceived, and (as our authors are careful to say) they who manufacture them deceivers, so far as the things are not uniform. So the inference from such things only comes to this, that uniformity is a character of manufactured articles when uniformity is part of the purpose of manufacture. Is then the new argument, after all, a special case of the old one? Not so: for when men produce as a novelty a special case of an old argument, this must be because it is an especially strong case of the same; but we have seen that the old argument owes much of its virtue to complexity and variety; therefore, our modern manufactured articles, which are above all things simple and uniform, will only furnish a special case of the old argument by furnishing an especially weak one. Design, in short, has nothing to do with the new argument, and we must look for analogies among manufactured articles which are uniform, not because uniformity adapts them to their purpose, but simply because they *are* manufactured articles.

The nearest approach I can think of is to be found on a scale almost molecular, for number and sometimes for magnitude, in a London wilderness of similar and similarly situated houses. It is oppressive to walk past these boxes so nearly identical in form, and to think of the infinite variety of their contents; to think how different they would have been, and how much fitter for their purposes, if their inhabitants could have secreted them as a snail secretes his shell. And why does it make all the difference that they have been manufactured? Why did not the manufacturer vary them according to the interests connected with them? Of course because he did not care about those interests; because he could not foresee them; and because it would not answer to try and provide for them. And now we understand the sort of manufacturer the new argument reveals: a manufacturer who does not care what becomes of his articles the moment he gets them off his hands by his formulas beginning to be interpretable; a manufacturer who cannot solve his own equations except in a grossly approximative fashion; a manufacturer who could not give his constants the proper values if he knew what values to give them.

Uniformity, in short, is not as such the sign of a manufactured article, except as it may be the sign of an imperfect manufacturer. I do not suppose this is what the new argument is meant to mean: but this, I submit, is what it does mean. Perhaps, however, some competent supporter of it will kindly explain it a little. C. J. MONRO

Yorkshire College of Science

WILL you permit a few words upon your allusion to this College in a leading article of the 8th inst.?

If its promoters have confined their present efforts to the establishment of a Faculty of Science, one cause has been that the amount of their funds compelled a selection instead of a comprehension of subjects. With a capital of 26,000*l.* they could not venture to cover so large a field as Owens College commenced upon with an invested endowment of four times the amount. But already, before our doors are opened, we have cheering signs that in providing a function to which endowments may be entrusted, the College will accrete to itself aid from widely-divergent quarters. The Royal assent has been given to an amended scheme of the Endowed Schools Commissioners for the Akroyd Charity, by which an important annual residue is allotted to the College, with representation upon the Trust. By the liberality of the Clothworkers' Company, the sum of 500*l.* per annum is set apart for three years for a Professor of Textile Industries and for Scholarships. Is it unreasonable to hope that new professorships will be established by the generosity of private individuals? The existence here of a flourishing School of Medicine is favourable to your views of massing the Faculties, and already a first link of union is being forged between the two bodies in relation to the classes in Chemistry.

Do not suppose that the College adopts *Pannus mihi panis* as its motto. A thoroughly practical community must run a risk of magnifying the *practice* of science rather than its *theory*, but if the selection of professors has been fortunate, there is no doubt that students will be taught practice through theory. Your forcible remarks will doubtless strengthen the hands of certain liberal donors to the College, who have offered increased sums when an Arts Faculty can be established.

Leeds, Oct. 12 R. REYNOLDS

On the Process of Tone-making in Organ-pipes

THE natural order of harmonic progression in an open organ-pipe is well known. That there is from the same pipe an inverse order of harmonics equally natural is not equally well known. There is no intimation that I am aware of, in any treatise on sound, of this fact having been observed, and the absence of recognition is no doubt attributable to a general disregard of the study of the comparative acoustics of musical instruments. My investigations into the process of tone-making in organ-pipes and other instruments have clearly shown me that there is *an order of transitive* harmonics distinct from the *order of concomitant* harmonics or "over-tones." Why I call them "transitive" will be apparent in the argument. Certain it is that our mimaphonic power in organ-pipes and in other musical devices depends on the command we can ensure over these two orders distinctively, and also on their comparative influences on the tones produced. In this manifestation of an inversion of harmonic progression, the nature, and, without extravagance one may say, the individuality, of the aëroplastic reed is most fully pronounced. Experimental proof is easily obtained, and, whilst bringing into prominence the peculiar display, will at the same time furnish indubitable evidence of the formative power exercised by the air-reed in the process of tone-making.

By the term "tone-making" is to be understood the manner of origination not merely of a note of defined pitch emitted by a musical instrument, but also of all the constituent sounds which give colour or quality to the note, and enter into the effect perceived by the ear. The artist, according to his sagacity, seizes